

the Chelifer is or is not a parasite on the house-fly. It is fully recognised now that house-flies play an important part in the distribution of the germs of certain diseases that affect mankind. Any animal, therefore, that injures or destroys the flies may assist in checking the spread of disease. But if, as Mr. Pocock suggests, the object of the Chelifer is to feed upon the acarine parasites of its host, it serves rather as a friend than a foe to the fly, and should certainly not be called a parasite.

There is no anatomical reason for believing that the Chelifers that have been found on flying insects are specially adapted to a parasitic mode of life, nor is there any evidence that the house-flies they are attached to are infested with mites or any other skin parasites. If the Chelifers are not parasitic on the flies, and there are no mites for them to attack, how can the association of the two forms be accounted for otherwise than by the transportation hypothesis?

Since I wrote my last letter to you I have found that this matter has been most fully discussed by Mr. Kew in his article on Lincolnshire Pseudoscorpions in the *Naturalist* for July, 1901, and I would refer readers of NATURE who are interested in the subject to that paper for fuller particulars.

SYDNEY J. HICKSON.

University of Manchester, October 21.

The Rudimentary Hind Limbs of the Boiné Snakes.

It is a well known fact that the pythons and boas and some allied forms among snakes possess rudiments of hind limbs, these vestiges—to quote Boulenger's "Catalogue of Snakes in the British Museum"—"usually terminating in a claw-like spur visible on each side of the vent." These structures are always mentioned in general works upon Ophidia, such as Hoffmann's account of the serpents in vol. vi. of Bronn's "Klassen und Ordnungen des Thierreichs," and Gadow's "Reptiles and Amphibians" in the "Cambridge Natural History." But in none of the three treatises to which I refer is there any further account of the "claws" or "spurs." It is merely stated that they are present. It is not mentioned in these works, nor in some others which I have consulted, that the claws in question offer valuable sexual characters by the aid of which individuals can be referred to their proper sex, at least in certain Boidæ. The fact that these characters have been so largely overlooked is perhaps due to the slight stress laid upon them by Duméril and Bibron (*Erpétologie Générale*, vol. vi., 1844), who, however, did direct attention to the occurrence of differences in these organs between the two sexes in a number of Boidæ. But they speak of the claws merely as being "d'une très petite dimension chez des femelles," and as "plus développés chez les mâles que chez les femelles." The first of these quotations refers to *Eunectes*, the second to *Boa*. The differences, however, in *Eunectes notaeus* are greater. In this southern anaconda, of which several specimens were lately deposited in these gardens by the Hon. Walter Rothschild, there is in the male a sharp curved claw turned downwards and ridged along its lower surface. In the female, on the other hand, the representative of this claw is not a claw at all strictly speaking—if, that is to say, we mean by a claw a nail-like structure which is curved and compressed and ends in a sharp point. In the female there is a straight, blunt, horny process distinctly unlike the sharp claw of the male. In two young examples of this anaconda, which are females, the same type of horny structure is found as in the adult female. In the allied genus *Eryx* there are still greater differences between the two sexes.

FRANK E. BEDDARD.

Zoological Society's Gardens, October 18.

A Rare Game Bird.

MR. SAWBRIDGE (p. 605) has raised one of the most perplexing points connected with bird-migration. I cannot answer for the eastern counties of England, but here, in the south-west of Scotland, we are still further from the headquarters of the quail than he is. Fifty years ago quails bred regularly in western Galloway; as a boy I recollect that two or three brace were quite a common complement to a September bag. Indeed, when a

"cheeper" or undersized partridge was shot, "Put it down as a quail!" was the usual comment. These birds gradually disappeared; the last that I myself shot was about the year 1868; but an odd one has been obtained here and there in the district ever since. One, I know, was shot last month in the neighbourhood of Newton Stewart, and was reckoned such a curiosity that it was sent to the bird-stuffer. I am sorry that I do not know whether it was a young or an old bird. Besides this, other instances, if I mistake not, have been recorded in the *Field* from different parts of the country.

As to the cause of the general disappearance of quails from this district, there have been many speculations, the commonest notion being that the supply is so heavily taxed in the Mediterranean region that few birds escape to the north. Truly, when one considers the enormous consignments of quails to London, Paris, &c., there is no reason for surprise that the migrants should dwindle in number.

I have a vague recollection of being told in boyhood that about the year 1838 there was a large influx of quails into Galloway, and that they had bred there ever since, but in numbers annually decreasing. It is conceivable that a storm-driven flock may have been carried out of their bearings, and, finding food abundant and climate endurable, if not altogether congenial, remained as colonists, but that our wet summers have proved adverse to their young being reared. The fluctuation in the stock of partridges caused by the character of different seasons is very remarkable, and evidently neither the numbers nor the constitution of our quails have enabled them to survive adverse conditions of temperature and rainfall. This makes the sporadic occurrence of individuals at long intervals all the more remarkable and perplexing.

HERBERT MAXWELL.

Monreith, Wigtownshire, October 22.

On a New Species of Guenon from the Cameroons.

A CHARMINGLY docile species of guenon, obtained by Cross, of Liverpool, from the Cameroons, in West Africa, and recently submitted to me for identification proves to be undescribed. I propose for it the name *Cercopithecus crossi*, in compliment to the courteous proprietor of that large and well known importing house of wild animals, and for popular use the same of Cross's guenon. The animal is a male, apparently nearly full grown, but not entirely adult, as the condition of its teeth indicate. It is very similar to *C. moloneyi* of Sclater, in general appearance, in having the broad rufous lower back, but differs in having a large and bushy pure white beard, white throat, and bushy whiskers of black hairs ringed with white; the band across the forehead deep black instead of fulvous; sides of head speckled black and white; underside of body sooty-black speckled with white; the tail not deep black except at tip, but speckled black and white like the upper part of the back; the black on the forearm externally does not extend to the shoulder, and not much beyond the elbow; the outer aspect of thighs is black slightly peppered with white; the inside of arms below the elbow black, higher up sooty-grey; inside of hind limbs sooty-black.

The top of the head is black, the hairs sparsely ringed with white; the face, cheeks, and ears quite nude and purplish black in colour; long superciliary hairs are present; the callosities are small and purplish sooty-grey in colour.

From *C. albigularis* (Sykes's guenon) the present species differs in wanting the yellowish wash on shoulders, fore and hind limbs, and in having a brindled and not a black tail.

HENRY O. FORBES.

The Museums, Liverpool, October 12.

The Absorption Spectrum of Benzene in the Ultra-violet Region.

WE were glad to see in NATURE of October 5 a letter from Prof. Hartley in which he points out the near agreement between our measurements of the bands in the absorption spectrum of benzene and those made by Prof. Dobbie and himself. He also directs attention to the work of Friederichs, who, in the case of benzene vapour,

finds the position of these bands to be consistently nearer to the red end of the spectrum. The difference in the position of the bands in the spectrum of benzene vapour and of benzene in solution only proves, of course, the applicability of Kundt's rule. We are also pleased that Prof. Hartley has been able to see the second band on our list ($\lambda=2656$), which, coupled with the fact that Friederichs has also measured it, we feel is a most important confirmation of our observations.

As regards the eighth band ($\lambda=2330$) which has been measured in the absorption spectrum of benzene vapour by Friederichs (whose work we were, of course, unaware of when we wrote our paper), we have made a most careful search for it. We have re-examined our original plates and have taken several more photographs, but have been unable to find any trace of it. We must therefore conclude that it is absent from the spectrum of benzene in alcoholic solution.

There is one other point in Prof. Hartley's letter; he says we have overlooked some points of importance in his paper with Prof. Dobbie when we state that they only found six bands. It is quite true that in their paper Hartley and Dobbie refer in their table of measurements to another band of very short persistence which they mark as doubtful at 5 mm. thickness of N/10 solution, and very doubtful at 4 mm. thickness. In the letterpress, however, they speak of only six bands, and in all later publications benzene is stated to show six absorption bands. In the British Association report, and even in Prof. Hartley's paper to the Chemical Society on May 17 of this year, he speaks of six bands (*Chem. Soc. Proc.*, xxi., 167). We therefore assumed that Prof. Hartley, on further consideration, had concluded that this doubtful band was not a true benzene absorption band. As we ourselves had seen no trace of this band, we in our paper before the Chemical Society (*Trans. Chem. Soc.*, lxxxvii., 1332) stated that Hartley and Dobbie had found only six bands.

Prof. Hartley's ideas and work upon the absorption spectra of organic compounds in the ultra-violet are of the greatest importance; he was the first to show how the constitution of certain compounds could be established by this means. Prof. Hartley's method of "testing" a molecule by means of its absorption spectrum, we are sure, will prove of the greatest possible value in the hands of chemists.

E. C. C. BALY.
J. NORMAN COLLIE.

University College, October 12.

Action of Radium Salts on Gelatin.

HAVING occasion to give a demonstration of the properties of radium some little time ago, I determined to attempt the preparation of some of the organisms as described by Mr. J. Butler Burke.

The method employed was to sprinkle a few specks of the radium salt upon the surface of some sterilised gelatin contained in a test-tube, and then to await development. That did not take long. Almost at once a faint cloudiness appeared to start under the speck of salt which extended downwards into the gelatin, in some cases after twenty-four hours reaching the depth of one centimetre. No heating was required to bring about this "growth," which resembled to the unaided eye an ordinary mould. The experiment was made with radium preparation of varying degrees of activity, but it was soon observed that the degree of activity in the salt had little influence on the growth, a salt of radium barium bromide containing 1/1000 of its weight of active salt being nearly as efficacious as one containing 1/100. (The more pure specimens which I possess were too precious to experiment with.)

As the specimens used were composed chiefly of barium salt, it occurred to me that it might be interesting to try the effect of the pure barium salts on the gelatin. This was done, with the surprising result that the "growths" were just as easily obtained as with the radium preparation—or even more so. I have tested all the barium salts at my disposal, and find the following produce the effect:—Barium, oxide, dioxide, chloride, bromide, iodide, nitrate, acetate, tartrate, and sulphovinate, while the phosphate,

carbonate, sulphate, and borate do not act. Thus the soluble salts are active, and the insoluble ones inactive.

The method adopted for the experiments was as follows:—Some clear gelatin was poured on to a glass slip and allowed to set. A tiny speck of the salt was placed on the gelatin and covered with a thin glass. This slip was then placed on the stage of a microscope and examined with a $\frac{1}{2}$ -inch power. At once the "growth" was seen to shoot out from the speck, and it appeared to consist of bubbles, some large, but most of them very small. Half an hour afterwards the speck had dissolved, leaving in its place a nebulous patch many times the size of the speck. The action of barium iodide is particularly rapid, while that of the hydrate is rather slow. I have tried uranium and thorium salts, both of which affect the gelatin rapidly, but do not produce the "growths." The action of these salts upon gelatin seems to point out an interesting field of inquiry, which I propose to follow.

W. A. DOUGLAS RUDCE.

Woodbridge School, Suffolk.

The Problem of "Shadow-bands."

SUBSEQUENTLY to the Algiers eclipse of 1900, it occurred to me that the "shadow-bands" visible at times of total solar eclipse might be merely another aspect of the "boiling" distortions of the sun's limb inseparable from daily observations. The last few years have therefore been employed by me in studying the characteristics of "boiling" with the view of making a direct comparison of evidences at the first opportunity. This opportunity presented itself in the recent total solar eclipse observed by me at Cás Catalá, in Mallorca, on August 30 last.

Employing "Carrington's method" of projecting the sun's image with a small telescope, the first observation made at about 10 a.m. recorded the existence of two distinct layers of cloud, the lower one travelling N.E. by S.W., and the upper one W.S.W. by E.S.E., giving confused and erratic "boiling." Further observations revealed an increased prevalence of the N.E. cloud system, but the drift from W.S.W. was still in evidence. At 11.35, however, it transpired that the W.S.W. system alone prevailed, and all trace of the drift from N.E. had abated. Continuing the observation without any relaxation throughout the phase of partial eclipse until within a few minutes of totality, I was able to ascertain that the "boiling" movements along the advancing limb of the moon were throughout absolutely in agreement in every particular with the movements of distortion affecting the still uncovered limb of the sun. Observations by projection were abandoned at 1h. 18.0m. for the purpose of securing a naked-eye view of "shadow-bands." A very successful view of these was secured. Their direction of flight determined on the spot, and afterwards corrected by Dr. Hunter, of Edinburgh, by the compass, proved to be W.S.W. by E.S.E. It is noteworthy that at Palma, where the eclipse conditions were marred throughout by the cloud bank that had threatened to overwhelm us at Cás Catalá (only four miles S.W. of Palma), the "shadow-bands" were observed to take a direction N. 30° E. by S. 45° W.

CATHARINE O. STEVENS.

Bradfield, Berks, October 20.

Rhymes on the Value of π .

Now I know a spell unailing,
 3 1 4 1 5 9
 An artful charm, for tasks availing,
 2 6 5 3 5 8
 Intricate results entailing.—
 9 7 9
 Not in too exacting mood,
 3 2 3 8 4
 (Poetry is pretty good),
 6 2 6 4
 Try the talisman.—Let be
 3 3 8 3 2
 Adverse ingenuity!
 7 9

π .